ture below C, such as t, the two substances on being mixed will form two solutions of the composition D and E respectively, whereas at and above C they will form but one homogeneous liquid, their mutual solubility being infinite. This figure is similar to that for aniline and water reproduced by Prof. Orme Masson in Fig. 1, p. 347.

In the above I have purposely avoided using the terms solvent and dissolved substance, since there is much confusion as to their meaning, and, indeed, it is perhaps impossible to differentiate them: when talking of the freezing-points, that substance which crystallizes from the liquid is generally termed the solvent, whereas, when talking of solubilities, the crystallizing substance is termed the dissolved substance.

The want of sound logic displayed in the arguments of the advocates of the gaseous theory of solution, must, I think, be a matter of surprise to many. Their chief argument is this: so-called osmotic pressure is (roughly and with many palpable exceptions) numerically equivalent to the gaseous pressure which the dissolved substance might be expected to exert if it could be gasified; *therefore*, the dissolved substance is a gas. They forget that the osmotic passage of water through a membrane in order to arrive at a solution on the other side, though it might be caused by the dissolved substance bombarding the membrane which it cannot penetrate, might also be caused by other means, such as an attraction of the solution for more water, or by the effective pressure of the solution being less than that of pure water. Surely before building up such a vast superstructure on the foundation of the gaseous nature of the dissolved substance, it would be well to see whether that foundation is real or imaginary. If the dissolved substance is truly gaseous, and if the solvent, as Prof. Orme Masson says, only "plays the part of so much space," then the dissolution of a gas at constant pressure in a liquid should neither evolve nor absorb heat, but this, I can confidently assert, is not the case: if, again, the so-called osmotic pressure is due to gaseous bombardment, it will be $\frac{1}{2}mv^2$, and we should be able to deduce it from independent measurements of m and v, whereas preliminary experiments of my own lead me to feel fairly certain that $\frac{1}{2}mv^2$ does not give the osmotic pressure. Surely these are fundamental points, the investigation of which should have been the first duty of the advocates of the theory. The flimsy nature of the foundation will account for the number of props which the building requires. The osmotic pressure is not a constant independent of the nature either of the solvent or of the dissolved substance, it is not a rectilineal function of the concentration, and the solvent most palpably does not act as "so much space." The theory has, consequently, to be bolstered up on the side of weak solutions by a supplementary theory of dissociation into ions (with all its inconsistencies), and on the side of strong solutions by the never-failing resource of the destitute—the disturbing influence of hydrates and molecular aggregations.

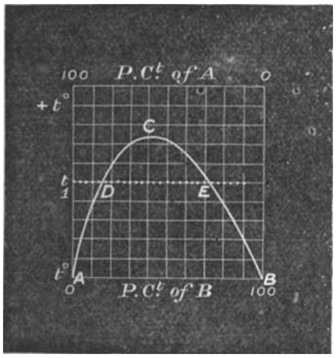

FIG. 2.

Van't Hoff started his theory by talking of osmotic phenomena as due to the attraction of the solution for more water. The proximate numerical equivalence of this attraction to gaseous pressure soon caused the substitution of "pressure" for "attraction." The latter has now been entirely lost sight of, and "pressure" has become a catch-word which has blinded his followers to the most patent facts, and has led them to press the supposed analogies of gaseous and osmotic pressure to the most absurd consequences. Prof. Orme Masson's address contains the latest development in this direction. He says, "Imagine, then, a soluble solid in contact with water at a fixed temperature. The substance exercises a certain pressure, in right of which it proceeds to dissolve. This pressure is analogous to the vapour pressure of a volatile body in space, the space being represented by the solvent; and the process of solution is analogous to that of vaporization." Now what can be the meaning of these sentences? What sort of pressure is it that a stable solid exercises? It is not ordinary vapour pressure, for that, where it does exist, does not render a solid soluble, and, indeed, we are told that it is only "analogous to vapour pressure," nor can it be osmotic pressure, for that is a property confined exclusively to (supposed) gases in solution; it can only be some novel property of solids which has yet to be revealed to an expectant world. This, I believe, is the only attempt which has been made to explain on the physical theory *why* a substance dissolves at all, why the solvent which, it is said, has no attraction for it and acts only as "so much space" should not only perform the mighty work required to liquefy and gasify a solid, but should also be able to retain it as a gas under enormous pressure; and if the physical theory is capable of giving no more satisfactory explanation of *the* fundamental fact of dissolution than the above, the sooner that theory is abandoned the better.

Harpenden, March 2. SPENCER U. PICKERING.

Co-adaptation.

THERE is one point in Prof. Meldola's review of Mr. Pascoe's book on the origin of species touching which it seems desirable that I should say a few words. The matter is introduced by the following passage:—

"Among the objections for which the author makes Dr. Romanes responsible is the well-known one about the giraffe:—'On the converting "an ordinary hoofed quadruped" into a giraffe, Mr. Romanes observes: "Thousands and thousands of changes will be necessary." . . . "The tapering down of the hind-quarters would be useless without a tapering up of the fore-quarters." The chances of such changes are "infinity to one" against the association of so many changes happening to arise by way of merely fortuitous variation, and these variations occurring by mere accident.' I cannot say how far this passage represents Dr. Romanes's views. The latter portion appears to contain a distinct pleonasm, but this is a point of detail, arising perhaps from the author having torn the passage from its context and then dissecting it."

The "dissected" sentences here referred to have been taken from an article on Mr. Wallace's "Darwinism," which I published in the *Contemporary Review* for August 1889. It is, perhaps, needless to say that the "pleonasm" does not occur in the original, and that I do not there hold myself responsible for enunciating Mr. Herbert Spencer's argument, which the quotation sets forth. I merely reproduced it from him as an argument which appeared to me valid on the side of "use-inheritance." For not only did Darwin himself invoke the aid of such inheritance in regard to this identical case, but likewise entertained such aid to natural selection as of "importance" in other cases where the phenomena of "co-adaptation" are concerned. Whether or not he underrated the power of natural selection in regard to such cases, it is in my opinion too early to dogmatize. But I am quite sure that "the well-known difficulty" in question cannot be met by the "Neo-Darwinians" with any appeal—explicitly or implicitly—to what is here the false analogy supplied by artificial selection. For example, suppose that there are n different parts which are required to vary, each in one particular way, but all to vary together in the same individual, if any of the variations is to confer an advantage in the struggle for existence. Suppose, further, that there is nothing but "chance" to lead to the simultaneous variation of all these parts in the same individual. Upon these data it is sufficiently evident that the happy combination would not occur with sufficient frequency to admit of being perpetuated in progeny—even if n be only equal to 4 or 5. Now I say that this "difficulty," be it great or small, cannot be met by what Mr. Wallace has called "the best answer"—namely, "the very thing said to be impossible by variation and natural selection has been again and again effected by artificial selection." For there is no "difficulty" at all in understanding how artificial selection is able to choose the separate congenital variations A, B, C, D, &c., as they severally occur in different individuals, and, by suitable mating, to blend

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them all in a single individual. Here the "selection" is *intentional;* and therefore the whole ground on which the "difficulty" stands is absent. This ground is the supposition of *fortuity*, with regard (*a*) to *all* the variations A, B, C, D, &c., happening to occur in any one individual to begin with, or (*b*) being afterwards preserved (by suitable mating) from obliteration by free intercrossing. Therefore, thus to appeal explicitly from natural selection to the analogy of artificial selection is to be cheated by a metaphor.

How, then, does it fare if the appeal be made implicitly, as in Prof. Meldola's review, by supplying *utility* in the one case as corresponding to *intelligence* in the other? Obviously, here again, the element of *fortuity* is ignored, and therefore, as previously, the "difficulty" is not met, but evaded. For no one who believes in natural selection could deny, that if *each* of the variations, A, B, C, D, &c., is of advantage *per se*, they would all be preserved as they severally happened to arise in this, that, and the other individual, till, by general intercrossing, they would eventually coalesce in single individuals—as in the case of artificial selection. But all this is quite wide of the mark. Indeed, intercrossing is here a necessary condition to, instead of a fatal impediment against, the blending of co-operative modifications; and therefore Mr. Spencer would have been a fool had he brought his "difficulty" to bear upon this case. This case, however, is not that which is meant by "co-*adaptation*": it is the case of a confluence of *adaptations*. Or, otherwise stated, it is not the case where adaptation is *first initiated in spite of intercrossing*, by means of a fortuitous concurrence of variations, each in itself being without any adaptive value; it is the case where adaptation is *afterwards increased by means of intercrossing*, on account of the blending of variations each of which has always been of adaptive value in itself.

The "difficulty," therefore, remains just where it was before; and the only way of meeting it is to show that the phenomenon of co-adaptation does not occur in nature. In other words, it must be shown that the difficulty is fictitious, by showing that, as a matter of fact, there are no cases to be found where *n* modifications, each being useless in itself, become useful in association. Whether or not the difficulty does admit of this the only rational solution, I will not occupy space by discussing; but I have thought it desirable to state what I have always understood to be the real nature of Mr. Spencer's "well-known objection."

Oxford, March 10. GEORGE J. ROMANES.

Neo-Lamarckism and Darwinism.

IT has been sometimes said that it is difficult to tell the difference between the supposed effects of the environment upon an organism, and the accumulation of favourable variations. There can be no difference; for they are but two explanations or theories to account for the same thing. A species is characterized by certain features; it is *these* which have to be accounted for; and any number of theories may be propounded as to the cause. It is simply a question as to which can be "proved" to be either the most probable or actually true.

At one time it was thought satisfactory to account for everything by a direct creative act. A man is exactly the same, whether he was created as he is, or evolved from animals; and if evolved, whether by the direct action of the environment or by natural selection or any other way. We may say with Burns, "A man's a man for a' that."

It is also said that the value of a theory depends upon the number of phenomena it can satisfactorily explain or account for. This is not altogether the case. The theory of creation accounted for everything; but we have abandoned it, nevertheless. The value of a theory really depends, not so much on what it can explain, as upon *the number of facts* on which it is based.

Now, are not many theorists forgetting the importance of this? I have just read Mr. Cockerell's paper on the "Alpine Flora" (NATURE, January 1), which will illustrate my contention. He has studied the flora of the mountains of Colorado, and finds that, as a whole, the plants are characterized by certain features. These are the same as are noticeable, not only on European and the Rocky Mountains, but in Arctic and Antarctic regions as well. He comes to the conclusion that "If this [lack of nourishment] were the only cause of dwarfing, the Alpine flora would present clear evidence for the transmission of acquired characters, as the character has undoubtedly become a *specific* one in several mountain plants." He here alludes to *one*, viz. a dwarf habit. The cause, however, which he gives is not the only one, nor is it in this case probably always the right one. If it were, then all mountain and all Arctic and Antarctic regions must have poor soils, for which there is no evidence. All these regions, however, have a relatively lower temperature.

Here, then, we have *two coincidences* of *universal application*—a dwarf habit and a low isotherm. Now we all know from experience how suddenly cold weather instantly checks growth in spring, &c.; therefore, we can infer, or draw the deduction, that the constantly low temperature of the Alps and Lapland perpetually check growth in those regions.

This alone would be a perfectly legitimate conclusion; as the probabilities of there being a distinct cause and effect underlying these coincidences are so great as to amount to a "moral conviction" of the truth.

Though this is logically sufficient, the deduction has been "verified by experiments." When seed is gathered from Alpine plants and sown at low altitudes,[1] and *vice versâ*, the plants raised after a few years begin to assume the characters, respectively, of the same species which are natives of the places. *Now* the argument is complete.

The preceding facts, therefore, warrant one in stating the theory thus: "That Alpine plants have acquired their special characteristics, by the responsive power of their protoplasm under the influence of their environment."

Having lived generation after generation under that same influence their characters have become relatively fixed, hereditary and "specific" as Mr. Cockerell believes. Such plants, however, probably never lose the power of changing again, as experiment shows.

To this scientific explanation Mr. Cockerell superadds the theory of natural selection. He endeavours to explain how natural selection "may" come into play as well. He says:—

"(1) They may escape the violence of high winds which prevail at those altitudes; taller plants being broken off before the seed matures."

Instead of appealing to facts, as he did before, he now begins with an hypothesis. Has he ever seen a taller plant broken off (as often occurs at lower altitudes)?

A mere suggestion is scientifically of little value, unless it be founded upon something which actually occurs.

"(2) They may obtain some additional warmth from their close proximity to the ground and partial shelter."

Here is a remark which ought to have been tested experimentally before being given to the world. Why should not a close proximity to the ground give a chill as well as, or instead of, warmth? As a fact, radiation at night begins on the ground, as the presence of hoar-frost tells us; and therefore we might ask, Are not dwarf plants just as likely (*a priori*), if not more so than tall ones, to suffer as well as to be benefited?

To what *facts* does "partial shelter" refer? Alpine plants are particularly exposed.

"(3) The short summer of the mountain tops necessitates very rapid development; and requires every energy to be thrown into the essential function of producing flowers and seed, leaving nothing to spare for the production of branched stems and diffuse foliage."

This seems like putting the cart before the horse; for how can a seedling plant know that the summer is going to be very short, and that it must, therefore, put forth all its energies? If it understood its own functions, it would know that the flowering depends entirely on the foliage; and, since M. Bonnier, for example, has shown[2] that the chlorophyllous tissue is increased in Alpine plants, this justifies us in looking to it as probably a sufficient cause of Alpine plants having fine flowers.

Finally, has Mr. Cockerell observed any plants which have "failed in the race, and so have been ruthlessly cut off by the autumn storms?" If so, will he give examples? If not, I would refer him to the paragraph italicised above.

To refer once more to the difficulty mentioned above. It must not be forgotten by those who feel that difficulty, that, while the action of the environment on plants is a thing which can be tested, and in many cases admits of easy proof by experiment, the accumulation of many useful variations which mark any living species must ever remain an *a priori* assumption, which is absolutely incapable of verification.

Cairo, February. GEORGE HENSLOW.

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[1] As by M. Bonnier, see ref. *infra*.

[2] *Bull. Soc Bot. de Fr.*, 1886, p. 467.

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